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ARTICLE I.

Outlines of Experiments and Inquiries respecting Sound and Light. By THOMAS YOUNG, M.D. F.R.S.

(Concluded from page 91.)

XIII. *Of the Vibrations of Chords.*

BY a singular oversight in the demonstration of Dr. Brook Taylor, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the Newtonian proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with an uniform velocity; and this velocity is the same which is inferred from Dr. Taylor's theorem; just as that of sound, determined by other methods, coincides with the Newtonian result.

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But, although several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of Euler and De la Grange, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an abscis each way, in the same proportion to the length of the chord as any given portion of time bears to the time of one semivibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of two right lines, as generally happens in musical instruments and experiments, its successive forms will be such as are represented in Plate VI. Figs. 47, 48: and this result is fully confirmed by experiment. Take one of the lowest strings of a square piano forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a miscroscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor Chladni, and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting some slight observations of Busse and Chladni, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Plate V. Fig. 49. But, if the chord be inflected near one of its extremities, Fig. 50, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from whence it was let go, but will be there faintly visible, on account of its short delay. In the middle of the chord, the excursions on each side the axis are always equal; and, beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates

vibrates at all: fully confirming the non-exiſtence of the harmonic curve, and the accuracy of the conſtruction of Euler and De la Grange. At the ſame time, as M. Bernouilli has juſtly obſerved, ſince every figure may be infinitely approximated, by conſidering its ordinates as compoſed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonſtrated, that all theſe conſtituent curves would revert to their initial ſtate, in the ſame time that a ſimilar chord bent into a trochoidal curve would perform a ſingle vibration; and this is in ſome reſpects a convenient and compendious method of conſidering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulſe, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate reſiſtance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or leſs ſimple and uniform according to circumſtances. Some ſpecimens of the figures of the orbits of chords are exhibited in Plate V. Fig. 44. At the middle of the chord, its orbit has always two equal halves, but ſeldom at any other point. The curves of Fig. 46, are deſcribed by combining together various circular motions, ſuppoſed to be performed in aliquot parts of the primitive orbit: and ſome of them approach nearly to the figures actually obſerved. When the chord is of unequal thickneſs, or when it is looſely tended and forcibly inflected, the apſides and double points of the orbits have a very evident rotatory motion. The compound rotations ſeem to demonſtrate to the eye the exiſtence of ſecondary vibrations, and to account for the acute harmonic ſounds which generally attend the fundamental ſound. There is one fact reſpecting theſe ſecondary notes, which ſeems intirely to have eſcaped obſervation. If a chord be inflected at one-half, one-third, or any other aliquot part of its length, and then ſuddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is intirely loſt, and is not to be diſtinguiſhed during any part of the continuance of the ſound. This demonſtrates, that the ſecondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any ſympathetic agitation of auditory fibres, nor upon any effect of reflected ſound upon the chord, but merely upon its initial figure and motion. If it were ſuppoſed that the chord, when inflected into right lines, reſolved itſelf neceſſarily into a number of ſecondary vibrations, according to ſome curves which, when properly combined, would approximate to the figure given, the ſuppoſition would indeed in ſome reſpects correſpond with the phænomenon related; as the coefficients of all the curves ſuppoſed to end at the angle of inflection would vaniſh. But, whether we trace the conſtituent curves of ſuch a figure through the various ſtages of their vibrations, or whether we follow the more compendious method of Euler to the ſame purpoſe, the figures reſulting from this ſeries of vibrations are in fact ſo ſimple, that it ſeems inconceivable how the ear ſhould deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which muſt be

extremely regular, and almoſt uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the aſſiſtance of a powerful magnifier. This difficulty occurred very ſtrongly to Euler; and De la Grange even ſuſpects ſome fallacy in the experiment, and that a muſical ear judges from previous aſſociation. But, beſides that theſe ſounds are diſcoverable to an ear deſtitute of ſuch aſſociations, and, when the ſound is produced by two ſtrings in imperfect uniſon, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exiſts. It muſt be confeſſed, that nothing fully ſatisfactory has yet occurred to account for the phænomena; but it is highly probable that the ſlight increaſe of tenſion produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickneſs or flexibility of different parts of the ſame chord, may, by diſturbing the iſochroniſm of the ſubordinate vibrations, cauſe all that variety of ſounds which is ſo inexplicable without them. For, when the ſlighteſt difference is introduced in the periods, there is no difficulty in conceiving how the ſounds may be diſtinguiſhed; and indeed, in ſome caſes, a nice ear will diſcover a ſlight imperfection in the tune of harmonic notes: it is alſo often obſerved, in tuning an inſtrument, that ſome of the ſingle chords produce beating ſounds, which undoubtedly ariſe from their want of perfect uniformity. It may be perceived that any particular harmonic is loudeſt, when the chord is inflected at about one-third of the correſponding aliquot part from one of the extremities of that part. An obſervation of Dr. Wallis ſeems to have paſſed unnoticed by later writers on harmonics. If the ſtring of a violin be ſtruck in the middle, or at any other aliquot part, it will give either no ſound at all, or a very obſcure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumſtance of the ſucceſſive impulſes, reflected from the fixed points at each end, deſtroying each other: an explanation nearly analogous to ſome obſervations of Dr. Matthew Young on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the correſponding harmonic is extremely loud; and the fundamental note, eſpecially in the loweſt harmonics, ſcarcely audible: the chord aſſumes the appearance, at the aliquot points, of as many lucid lines as correſpond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Plate VI. Fig. 51. According to the various modes of applying the bow, an immenſe variety of figures of the orbits are produced, Fig. 45, more than enough to account for all the difference of tone in different performers. In obſervations of this kind, a ſeries of harmonics is frequently heard in drawing the bow acroſs the ſame part of the chord: theſe are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowſtring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would ſeem, that the bow takes effect on the chord but at one inſtant during each fundamental vibration. In theſe experiments,

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the bow was ſtrung with the ſecond ſtring of a violin: and, in the preparatory application of reſin, the longitudinal ſound of Chladni was ſometimes heard; but it was obſerved to differ at leaſt a note in different parts of the ſtring.

XIV. *Of the Vibrations of Rods and Plates.*

Some experiments were made, with the aſſiſtance of a moſt excellent practical muſician, on the various notes produced by a glaſs tube, an iron rod, and a wooden ruler; and, in a caſe where the tube was as much at liberty as poſſible, all the harmonics correſponding to the numbers from 1 to 13, were diſtinctly obſerved; ſeveral of them at the ſame time, and others by means of different blows. This reſult ſeems to differ from the calculations of Euler and Count Riccati, confirmed as they are by the repeated experiments of Profeſſor Chladni; it is not therefore brought forward as ſufficiently controverting thoſe calculations, but as ſhowing the neceſſity of a reviſion of the experiments. Scarcely any note could ever be heard when a rod was looſely held at its extremity; nor when it was held in the middle, and ſtruck one-ſeventh of the length from one end. The very ingenious method of Profeſſor Chladni, of obſerving the vibrations of plates by ſtrewing fine ſand over them, and diſcovering the quieſcent lines by the figures into which it is thrown, has hitherro been little known in this country: his treatiſe on the phænomena is ſo complete, that no other experiments of the kind were thought neceſſary. Glaſs veſſels of various deſcriptions, whether made to ſound by percuſſion or friction, were found to be almoſt intirely free from harmonic notes; and this obſervation coincides with the experiments of Chladni.

XV. *Of the Human Voice.*

The human voice, which was the object originally propoſed to be illuſtrated by theſe reſearches, is of ſo complicated a nature, and ſo imperfectly underſtood, that it can be on this occaſion but ſuperficially conſidered. No perſon, unleſs we except M. Ferrein, has publiſhed any thing very important on the ſubject of the formation of the voice, before or ſince Dodart; his reaſoning has fully ſhown the analogy between the voice and the *voix humaine* and regal organ-pipes: but his compariſon with the whiſtle is unfortunate; nor is he more happy in his account of the falſetto. A kind of experimental analyſis of the voice may be thus exhibited. By drawing in the breath, and at the ſame time properly contracting the larynx, a ſlow vibration of the ligaments of the glottis may be produced, making a diſtinct clicking ſound: upon increaſing the tenſion, and the velocity of the breath, this clicking is loſt, and the ſound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be diſtinguiſhed two octaves below the loweſt A of a common baſs voice, conſiſting in that caſe of about 26 vibrations in a ſecond. The ſame ſound may be raiſed nearly to the pitch of the common voice; but it is never ſmooth and clear, except perhaps in ſome of thoſe perſons called ventriloquiſts. When the pitch is raiſed ſtill higher, the upper orifice of the larynx, formed by the ſummits of the arytænoid cartilages

cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrogade falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, two or three times as long in a falsetto as in a natural voice; hence, too, the difficulty of passing smoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the sound is acute: but this elevation is only necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibration, or according to Euler's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed, by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least four harmonic sounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed, in a late dissertation of M. Knecht, published in the musical newspaper of Leipsic. Perhaps, by a close attention to the harmonics entering into the constitutions of various sounds, more may be done in their analysis than could otherwise be expected.

XVI. *Of the Temperament of Musical Intervals.*

It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The Aristoxenians and Pythagoreans were in some sense the beginners of the controversy. Sauveur has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. Smith has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it. Kirnberger, Marpurg, and other German writers, have disputed with great bitterness, almost every one for a particular method of tuning. It is not with any confidence of success, that one more attempt is

made, which refts its chief claim to preference, on the fimilarity of its theory to the actual practice of the beft inftrument-makers. However we eftimate the degree of imperfection of two tempered concords of the fame nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate fum; for inftance, the imperfection of a comma in a major-third, occafions it to beat very nearly twice as faft as that of half a comma. If indeed the imperfection were great, it might affect an interval fo materially as to deftroy its character; as, in fome methods of temperament, a minor third diminifhed by two commas approaches more nearly to the ratio 6 : 7, than to 5 : 6; but, with this limitation, the fum of harmony is nearly equal in all fyftems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compofitions which are to be performed on an inftrument, it would be of no great confequence, to the fum of the imperfections, among which of the thirds they were divided: and, even in this cafe, the opinion of the beft practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of confiderable advantage in the general effect of modulation. But, when it is confidered, that upon an average of all the mufic ever compofed, fome particular keys occur at leaft twice as often as others, there feems to be a very ftrong additional reafon for making the harmony the moft perfect in thofe keys which are the moft frequently ufed; fince the aggregate fum of all the imperfections which occur in playing, muft by this means be diminifhed in the greateft poffible degree, and the diverfity of character at the fame time preferved. Indeed, in practice, this method, under different modifications, has been almoft univerfal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpofe may be anfwered, by making C : E too fharp by a quarter of a comma, which will not offend the niceft ear; E : G*, and A♭ : C, equal; F* : A* too fharp by a comma; and the major thirds of all the intermediate keys more or lefs perfect, as they approach more or lefs to C in the order of modulation. The fifths are perfect enough in every fyftem. The refults of this method are fhown in Table XII. In practice, nearly the fame effect may be very fimply produced, by tuning from C to F, B♭, E♭, G*, C*, F* fix perfect fourths; and C, G, D, A, E, B, F*, fix equally imperfect fifths, Plate VI. Fig. 52. If the unavoidable imperfections of the fourths be fuch as to incline them to fharpnefs, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other fide. An eafy method of comparing different fyftems of temperament is exhibited in Plate VI. Fig. 53, which may eafily be extended to all the fyftems that have ever been invented.

Table

Table XII.

A		B		C	
C	50000	1 C	+ .0013487	1 A, E	— .0023603
B	53224	2 G, F	.0019006	2 D, B	.0029122
B^b	56131	3 D, B^b	.0024525	3 G, F*	.0034641
A	59676	4 A, E^b	.0034641	4 C, C*	.0044756
G*	63148	5 E, A^b	.0044756	5 F, G*	.0049353
G	66822	6 B, C*	.0049353	6 B^b, E^b	.0053950
F*	71041	7 F*	.0053950		
F	74921	D			
E	79752				
E^b	83810				
D	89304	1 E^b, G*, C*, F*	— .0000000		
C*	94723	2 F, B^b, E, B	.0004597		
C	100000	3 C, G, D, A	.0011562		

A, shows the division of a monochord corresponding to each note, in the system proposed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few steps only, in the investgation of some very obscure but interesting subjects. As far as I know, most of these observations are new; but, if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but, should further experiments tend to confute any opinions that I have suggested, I shall relinquish them with as much readiness as I have long since abandoned the hypothesis which I once took the liberty of submitting to the Royal Society, on the functions of the crystalline lens.

I am, &c.

Emanuel College, Cambridge, THOMAS YOUNG.
8th July, 1799.

EXPLANATION OF THE FIGURES.

(See Plates IV. V. and VI.)

Plate IV.

Figs. 1—6. The section of a steam of air from from a tube .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in Fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs.

Figs. 7—12. A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with two gages, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that, where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in Fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20. A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with two water gages. The pressure was in Fig. 13, .1. Fig. 14, .2. Fig. 15, .5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, show the result of the observations with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 21—23. A, the half section of a current from a tube .3 in diameter, with a pressure of .5, of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

Fig. 24. The appearance of a stream of smoke, forced very gently from a fine tube. Fig. 25 and 26, the same appearance when the pressure is gradually increased.

Fig. 27. See Section III.

Fig. 28. The perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Figs. 29—33. See Section X. *

Fig. 34. The combination of two equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 35, 36, 37, a similar representation of a major third, major tone, and minor sixth.

Fig. 38. A fourth, tempered about two commas.

Fig. 39. A vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7.

Fig. 40. A vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

* By some oversight the figures 29 to 40 inclusive are not inserted in the Engravings. They shall appear in our next.---N.

Plate V.

Figs. 41 and 42. Two different positions of a major third, composed of similar vibrations, as represented by figures of sines.

Fig. 43. A contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of two equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of two equal sounds, derived from figures of sines. D, a musical consonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal sounds.

Fig. 44. Various forms of the orbit of a musical chord, when inflected, and when struck.

Fig. 45. Forms of the orbit, when the sound is produced by means of a bow.

Fig. 46. Epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations.

Figs. 47 and 48. (in Plate VI.) The successive forms of a tended chord, when inflected and let go, according to the construction of De la Grange and Euler.

Figs. 49. The appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts.

Fig. 50. The appearance of a vibrating chord, when inflected at any other point than the middle.

Plate VI.

Fig. 51. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 52. The method of tuning recommended for common use.

Fig. 53. A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into twelve equal parts, representing the semitones of the equal temperament described by Zarlino, differing but little from the system of Aristoxenus, and warmly recommended by Marpurg and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the eleventh method in Marpurg's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of Kirnberger and Sulzer; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the system of mean tones, the *sistema participato* of the old Italian writers, still frequently used in tuning organs, approved also by Dr. Smith for common use. S shows the result of all the calculations in Dr. Smith's harmonics, the system proposed for his changeable harpsichord, but neither in that nor any other form capable of practical application.

II.

II.

On the Means of foretelling the Character of the Summer Season, and the Benefits to be expected from the Cultivation of Grasses which vegetate at low Temperatures. In a Letter from Dr. THO. BEDDOES.

To Mr. NICHOLSON.

SIR,

MANY years ago it occurred to me, that the general nature of our summers might be anticipated in the spring, and sometimes in winter. I have frequently hazarded a prediction, and I begin to acquire some confidence in my rule.

It is founded on the effect of melting substances, particularly snow, upon temperature. In applying this fact to meteorology, I say, *if there be an unusual fall of snow in the countries to the N. and N.E. our summer,* cæteris paribus, *will be cold and wet.* In the winter of 1798 and 1799, I observed in the public papers frequent mention of immense falls of snow in these hyperborean regions; and you know what our summer was. From the best information I could procure, I collected the contrary during the two last winters; and I prophesied accordingly.

I do not take the word *summer* in the strict astronomical sense, otherwise it would be too soon to talk of that of 1801; and still the approaching season may seem to put my system to shame. But observe, Sir, that I do not pretend to be acquainted with all the different causes of atmospherical refrigeration; and I only point out one, the most decisive perhaps that exists, but still capable of counteraction, and for which there may be a substitute when it is not present.

Some of your readers may perhaps think of trying what I advance by a reference to meteorological tables. But you will remark, that they do not supply the requisite data. This is at least the case with those to which it is in my power to have access; and I wish any hint from me may occasion a fact so essential to us, to be annually ascertained as far as it can be done with accuracy. The snow that may fall in England is of small account; though I suppose an unusual fall here may often accompany an unusual fall in the countries from Jutland to Tobolsk.

I shall now trouble you with another winter speculation, that may lead to experiments in future beneficial. In mild winters there can, I suppose, be no doubt of the vegetation of the grasses. It has often appeared to me, that it goes on unequally in different grasses; and I believe I could name some which grow at a temperature not greatly exceeding 40°. However, I shall content myself with proposing it to persons of more leisure, to ascertain by actual weight and measure, *what is the constitution of the different individuals of this family*

with

with regard to temperature. If certain grasses of good quality should grow at a temperature lower than that at which others will grow, it would evidently be desireable to have winter pastures and water-meadows of such grasses.

It would likewise be desireable to extend these experiments to our highest summer heat, with due attention to moisture and light.

I am, SIR,

Your's respectfully,

THOMAS BEDDOES.

Clifton, June 9, 1801.

III.

Analysis of Manachanite from Botany Bay. By RICHARD CHEVENIX, *Esq. F. R. S. and M. R. I. A. Communicated by the Author.*

ABOUT three months ago, I received from the Right Honourable President of the Royal Society, a quantity of sand, which had been found on the sea coast in Providence Island, and thence brought over to England in the course of the winter. It may not be very easy to determine the mineralogical character of any substance in the state of fine powder; but, from its general appearance, the object of our present inquiry was judged to be Manachanite. It was in fine grains, almost as small as sea sand; it was mixed with many white, and some red globules of the same size with the black; and, in this alone, differed from the manachanite of Cornwall. The white globules seemed to be quartz; and the red, as far as could be judged, appeared to be garnet, more than ruby. The specific gravity of so fine a sand, I did not think a character more certain than any other; therefore did not give myself much trouble to take it with accuracy. From the rough experiment I made, it did not appear to be very different from that of the known manachanites. It was slightly magnetic also; but not satisfied with any external marks, I proceeded to try what information I could receive from chymical analysis.

100 parts of the above sand from Providence Island, were exposed to a red heat for half an hour, and weighed upon cooling. No sensible augmentation or diminution of weight had taken place.

Sulphuric, nitric, muriatic, and nitro-muriatic acids, were heated for twelve hours upon a given quantity; and, upon trial with various re-agents, exhibited no marks of having operated an effectual solution.

1. I then took one hundred grains, and treated them with pot-ash, in a platina crucible, in the usual manner of treating hard stones. In about two minutes after the pot-ash was in fusion,

fufion, the mafs became brownifh, and quickly changed to a rich emerald green. After keeping it red hot for half an hour, the crucible was withdrawn from the fire, and the mafs diluted in water. The liquor, not without fediment, was of a very beautiful green, as is ufual in foffils, where iron is abundant, and but little oxidated.

2. Muriatic acid was then poured in, and a total folution enfued. This liquor was fet on a fand heat to evaporate at a moderately elevated temperature. After fome time, a white matter, much like curdled milk, began to appear, and afterward increafed to a confiderable volume. This fubftance, feparated by filtration, was well wafhed, and the liquor again expofed to the fame temperature was evaporated to drynefs.

3. Upon pouring in a large quantity of diftilled water to re-diffolve the mafs, a precipitate, which was filica, remained behind; but the liquor paffed very turbid through the filtre, although repeated attempts were made to render it limpid by that method.

4. A folution of pot-afh was poured in excefs into this turbid liquor, a red and confiderable precipitate fell to the bottom. The whole was made to boil; but no diminution took place in the apparent volume of the precipitate. The liquor was filtered, and effayed for alumina; but the pot-afh did not appear to have diffolved any thing.

5. The red precipitate was again diffolved in muriatic acid, and effayed by the known re-agents, for the different earths and metals. From thefe trials it appeared to be oxide of iron, mixed with a fmall portion of the fame fubftance as that which had coagulated in the firft attempt to evaporate the intire folution, as above ftated.

6. I now proceeded to examine the nature of the white fubftance (Exp. 2d.) which had feparated itfelf by fpontaneous precipitation. I attempted to re-diffolve a portion of it in muriatic acid, but could not fucceed. Nitric acid was much more effectual; but fulphuric acid, after being gently heated with it, operated a perfect folution. Some trials, made with the different re-agents upon the latter folution, afforded the following refults:

With pot-afh, foda, and ammonia, a white precipitate; with carbonates of pot-afh, of foda, and of ammonia, ditto; with a muriate or a nitrate, no precipitate; with an oxalate, a white precipitate; with a tartrite, a white precipitate; with a phofphate, a copious white precipitate; with an arfeniate, a ditto ditto; with fulphurated hydrogen water, no precipitate; with an alkaline hydro-fulphuret, a dirty glafs green precipitate; with a pruffiate, a dark blueifh green, which fhewed that there was ftill fome iron; with tincture of galls, a blood red, and very thick precipitate; with phofphoric acid, a white precipitate; with arfenic acid, a white precipitate.

Thefe experiments, and particularly the laft three, were fufficiently characteriftic, and proved that this fubftance could be no other than oxide of titanium. However, it was ftill contaminated with iron; and a part of it appeared to have remained with that red precipitate, which was already mentioned. To effect a complete feparation was ftill a *defideratum*. For this purpofe, I repeated the experiments upon different portions of the fand. After fufion with pot-afh, and folution in muriatic acid, I attempted to precipitate the whole, and then to diffolve in an excefs of carbonate of ammonia, the oxide of iron, become

3 red

red by the various treatments. But although red oxide of iron is eafily soluble in this salt, it is not sufficiently so to answer the purpose, when oxide of titanium is present. Such instances of the inefficacy of methods, as analytic means, although the primitive facts upon which they are founded are rigidly true, are frequent in chymistry; and indeed it may almost be said, that no two substances come in contact, without producing a reciprocal change in their properties. Hence we must be contented to make slow progress in the art of analysis; and perhaps there may be an inherent principle in the chymical nature of all bodies, that will long impede our way to absolute precision. I was therefore obliged to renounce this method.

I had perceived that phosphoric and arsenic acids formed a precipitate in sulphate of titanium, while neither of these acids produced any alteration in sulphate of iron. I therefore precipitated, by phosphoric acid, an intire solution of the sand in sulphuric acid, and by that means operated as good a separation of the oxides of iron, and of titanium, as is generally obtained in chymical processes. But we are ignorant of the quantity of titanium contained in salts of this metal, or in its oxide; for as yet it has never been obtained in sufficient abundance, reduced to the metallic state; and we have not been able to determine the proportion of oxigen that it may take up in its different stages of oxidation. But I had found about eleven of silica, and forty-nine of black oxide of iron. The remainder must therefore be oxide of titanium, and the proportions in the quintal will stand thus:

Silica - - - -	11
Oxide of iron - -	49
Oxide of titanium - -	40
	100.

proportions which differ but little from the Cornish manachanite. Some allowance, however, should be made for the inevitable loss.

The degree of fire necessary to operate a perfect reduction of titanium, exceeds what we have been able to produce. At least neither Mr. Klaproth nor Mr. Vauquelin appears to have succeeded in obtaining any large quantity.

I have never been so fortunate as to obtain this metal reduced and pure, except in small spicula, disseminated through the flux I had used; therefore not in a state fit to be subjected to experiment; but I have obtained it, combined with another substance, and without the aid of any alloy. I mixed with charcoal some phosphate of titanium, and, having added a flux of borax, put it into a double Hessian crucible well luted, and exposed the whole to a violent heat in a forge, into which the wind was impelled through four holes, and by two pair of bellows heavily loaded. After giving at first a gentle heat, increasing it by degrees for three quarters of an hour, and then suddenly raising it as high as I could raise it by the above apparatus, and maintaining at the same degree for half an hour, the crucible was withdrawn and broken. The upper crucible, which served as cover to the whole, was lined with a shining metallic coat, and a metallic button was found in the

bottom.

bottom. This button was phosphuret of titanium; it was of a pale white, not devoid of lustre. It was easily broken, and its fracture was grainy and white. It did not melt before the blow pipe, and seemed not very fusible. Muriatic and sulphuric acids did not attack it, nor did nitric acid very easily when cool; nitro-muriatic acid had much more effect upon it, and the liquor gave traces of titanium, and of phosphoric acid.

Phosphoric and arsenic acids were observed by Mr. Klaproth, when he discovered this metal, to precipitate oxide of titanium from the other acids, by direct affinity. However, an excess of muriatic acid can dissolve phosphate and arseniate of titanium; but sulphuric, or any other acid I have tried, has not that property.

In order to obtain oxide of titanium totally free from every other metal, with which it is accompanied in nature, such as iron or manganese, either of the above acids may be used, and that precipitate treated with fixed alkalis, or their carbonates.

Mr. Vauquelin has given a very ingenious explanation of the reason, for which nitric and muriatic acids when heated, cannot retain the oxide of titanium in solution. With the former, it is because the oxide takes a new portion of oxigen from the nitric acid, and becomes too much oxidated to remain in solution. With the latter the reverse is the case, and oxigenated muriatic acid is disengaged. However, in evaporating a large portion of the solution of this sand, which had been effected by the means of pot-ash, and muriatic acid, as above stated, I obtained at the end a salt well crystallized, which, upon strict examination, exhibited all the properties of oxigenated muriate of pot-ash. Hence it is evident that the oxigen had passed, probably in its nascent state, into the muriate of pot-ash already formed; for the first portion of muriatic acid, and long before a total solution of the sand could have taken place, must, by the laws of affinity, have gone to saturate the pot-ash employed. I have attempted to produce a similar combination by the means of manganese, but without success. The circumstances are not indeed rigidly similar; for in one case we effect a solution, and in the other, a precipitation of the metal that gives out the oxigen, and that may produce some modification.

If the mineralogical riches of the country, from which this sand was brought, are at all to be prejudged, from the valuable productions with which we have become acquainted in the vegetable and animal kingdoms, we may look forward to many interesting discoveries; and already, a metal is found to be a native of those climates, which has not long been known, and has been but rarely met in Europe.

IV.

IV.

A Series of Experiments upon Metals with an Electrical Battery, ſhewing their Property of abſorbing the Oxigen from the Atmoſphere when exploded by Electric Diſcharges. By JOHN CUTHBERTSON, *Philoſophical Inſtrument Maker, No.* 54, *Poland Street, London. Communicated by the Author.*

THE diſcovery that metals could be fuſed by electric diſcharges, gave riſe to a ſuppoſition that they might alſo be converted into oxides of the ſame means. Many attempts have been made to aſcertain this, but the fact has hitherto remained without proof. In the year 1787, Dr. Van Marum and myſelf produced floculi from different metals by ſubjecting them to ſtrong electric diſcharges, and alſo formed beautiful figures upon paper by exploding metallic wires extended at a ſmall diſtance from its ſurface. We imagined that the floculi, as well as the coloured figures upon the paper, were oxides of the metals we made uſe of; and in order to prove this, we entered upon a courſe of experiments. But having, perhaps from improper management, the misfortune to break ſeveral glaſſes in the proceſs, Dr. Van Marum declared himſelf ſo much diſcouraged by theſe accidents, as to decline proſecuting the ſubject.* When we conſider, however, the opulence of the ſociety of which he was director, it is not eaſy to conceive that ſo trifling an accident as the fracture of a few glaſſes ſhould be a ſufficient reaſon for his relinquiſhing this inveſtigation, more eſpecially as he had the command of an electrical apparatus which I made for that ſociety, not only the moſt proper for that purpoſe, but unequalled in the whole world, and from which I now fear we have little to expect.

In 1793—1794, I undertook two different courſes of experiments at Amſterdam upon the ſame ſubject; but both ended unſatisfactorily, and afforded very little more information than we before poſſeſſed. †

On my return to London, after delivering a courſe of lectures upon electricity, I once more returned to the above-mentioned ſubject. My previous experiments had taught me that it was neceſſary to have an apparatus into which atmoſpherical air or gas could be introduced, and any deſirable quantity of metallic wire, ſo that it would be exploded at pleaſure, and the diminution of the air or gas accurately meaſured. I invented the following apparatus, which poſſeſſed all the requiſites.

* Second Continuation des Experiences faites par la Moyen de la Machine Electrique Teglerienne par Martinus van Marum à Haarlem, page 272.

† Eigenſchappen der Electricitiet 3de. deel blad. 2, 143. Amſterdam, 1794, door John Cuthbertſon.

Deſcription

Defcription of an Apparatus in which Metallic Wires are converted into Oxides by Electric Difcharges.

Fig. 11, Plate VII. *a b*, *c d* is a glafs receiver, about ten inches high, and fix inches diameter. To the wideft end is cemented a large round brafs plate, *a b*, perfectly air tight, the other end is mounted with a brafs collar, upon which a brafs cap, *p q*, is fcrewed, alfo perfectly air tight. *f* is an air cock, one end of which is fcrewed into the middle of *p q*, and the other into a foot on which the inftrument ftands; *n o* are two air cocks fcrewed faft to the broad brafs plate, with holes communicating with the infide of the receiver. Upon each of thefe cocks is fcrewed a glafs tube bent, as is reprefented; their communication with the infide of the receiver is opened or fhut by means of *n* and *o*. In the infide of *p q* is a wheel and axis moving upon two pivots, ferving to wind up a quantity of wire in readinefs for exploding. *m* is a brafs tube about three inches long, and three quarters of an inch in diameter, ftopped at each end with cork, and the middle filled with hogs'-lard; this ferves to draw the wire through ready for exploding.

The wire which is intended for ufe, muft be bound to a packthread of the fame length, at different places, and at about four inches afunder; it muft be wound upon the axis of the wheel in *p q*; the tube *m* muft be fcrewed off from the plate, and the end of the wire muft be directed through the hole wherein *m* was, by means of a long brafs needle, which muft be pufhed through the cork and hogs'-lard, in *m*; with the packthread and wire in the eye of the needle; when it is drawn through, the packthread and wire will follow, and be kept air tight by the hogs'-lard and cork; *m* muft be fcrewed to the plate as before, and the packthread drawn through *m* till it is feen nearly ftraight in the infide of the receiver, as at *h*. In this ftate, pour into the lower bend of *k* about half of an inch of mercury, and in the lower bend of *i* a little water, reprefented by the dots in each. It is evident, from the conftruction of the inftrument, that if all the fcrews be perfectly air tight, the cock *f* fhut, and the cocks *n o* open, the leaft alteration that happens to the air in the infide of the receiver, with refpect either to encreafe, decreafe, denfity, or rarefaction, will be fhewn either by the mercury or water, in either one or both gages; if confiderable, the mercury will fhew it, and if too little to affect the mercury, the water gage will fhew it: if by any procefs the temperature of the air within the receiver fhould be raifed, it will caufe the air to expand, and the mercury and water in the gages to move towards *u u*, but if it fhould be lowered, the mercury and water will move towards *i k*; fo likewife if any alteration, with refpect to temperature, happens in the furrounding air, the gages will fhew it accordingly; and as this is continually changing, it is neceffary that the apparatus, after is prepared for the experiment, and ready for exploding of the wire, fhould be fet in fome medium which is not fo changeable. For this purpofe water feems to be the moft convenient; but as it is a conductor of electricity, the apparatus muft not remain in when the difcharge is given through it; fo that before, and after the difcharge, it muft only

be ſet into the water, care being taken to let it remain a ſufficient length of time that the air in the inſide may become of the ſame temperature, which will be ſhewn by the gages being ſtationary. If this circumſtance be not obſerved with great attention, errors in the concluſion will be the conſequence.

EXPERIMENT I.

A ſufficient quantity of leaden wire, of $\frac{1}{90}$th part of an inch diameter, was bound to a cord at different places about four inches diſtant, as has been already mentioned, and wound upon the axis of the wheel in *p q*, the end of it, by means of the long braſs needle, was drawn out at the top of the receiver through the tube *m*, which was ſcrewed in its place, and the cord drawn tight, as repreſented at *h*. The cock *f* was then cloſed, and the whole inſtrument ſet in the water ſo deep, that the braſs plate was about one inch under the ſurface of the water. The cocks *n o* were opened to admit the external air into the receiver, as the air within became condenſed by the coldneſs of the water. When it had remained nine minutes under the water, the gages were ſcrewed on to the cocks perfectly air tight, and both appearing ſtationary, the cocks were ſhut. The inſtrument was then taken out, wiped dry, and ſet as near the electrometer, Fig. 2, as poſſible, not to influence the motion of the ball A, namely, at the diſtance of about one foot. One end of a wire, *a b*, was hooked to the cock *n*, and the other to the hook of the electrometer.* One end of another braſs wire, *s t*, was hooked to the cock *f*, and the other end made to touch the outſide of the battery, Fig. 1, which is compoſed of fifteen jars, containing about ſeventeen ſquare feet † coating, and connected with the electrometer by means of a wire L. The battery was charged till the ball A of the electrometer, which was loaded with twenty-ſix grains, deſcended and directed the diſcharge through the wire *h*, which was converted into a blueiſh white impalpable powder, at firſt appearing like a white cloud, and filling the whole receiver (the cord was left entire); the inſtrument was then releaſed from the electrometer, and ſet in the water as before. After it had remained there five minutes, the cocks under the gages were opened. No change could be perceived in the mercury gage, but a motion in the water gage was juſt perceptible. Hence I concluded that the powder thus produced had abſorbed no air.

EXPERIMENT II.

The apparatus was taken out of the water, and another length of wire drawn forward, and placed at the electrometer and battery as before; the battery was charged till the electrometer cauſed the diſcharge, which again produced the powder. The apparatus was not put into the water as before, but was left to remain till another length of wire was drawn forward, and the diſcharge directed through it. And this proceſs was repeated in the

* For a deſcription of this electrometer, ſee this Journal, II. 528.

† For an account of this battery, ſee Journal, II. 526.

whole

whole by five discharges through five lengths of wire, each length was seven inches. In this manner, therefore, thirty-five inches of wire was converted into a blueish white impalpable powder. The apparatus was then placed in the water as before, and after it had remained the same length of time, the mercury gage was opened. No motion in the mercury was perceived, and when the water gage was also opened, no alteration was there seen. Hence I concluded that no absorption of air had taken place, though five times the original length of wire had been reduced to powder. The air in the apparatus was then eudiometrically examined, and it appeared to be of the same purity as the surrounding atmosphere.

EXPERIMENT III.

The apparatus was taken out of the water, and the powder which was produced in the receiver carefully taken out; the apparatus was then furnished with the same length of wire as before, all the screws and cocks were well examined, and the apparatus set in the water as before. After it had stood its time, the gages were screwed tight, it was taken out of the water and set to the battery when the last experiment was repeated, and it was afterwards set in the water, and the gages opened. No absorption having taken place, it was left in the water till the same hour on the following day, when the mercury gage was opened, and indicated that an absorption of about one cubic inch had taken place. The water was of the same temperature as at first. This experiment consequently shews that the metal was first divided, and afterwards oxigenated by a slow process.

EXPERIMENT IV.

The last experiment was repeated with double the length of wire, and the result was the same.

EXPERIMENT V.

A smaller receiver was now taken, being only four inches diameter, which was half the diameter of the former. The same kind of powder was produced, and a diminution of air happened when thirty-five inches had been reduced to powder, which caused the water gage to rise one inch, and the mercury $\frac{1}{20}$ inch.

EXPERIMENT VI.

Wires of different diameters were now prepared in the last receiver, of $\frac{1}{100}$ and $\frac{1}{140}$ inch diameter, and of each thirty-five inches; and the result was the same with respect to air, but the powder was whiter.

By the result of the three last experiments, it appeared that the absorption of air by the exploded metal apparently depended upon the diameter of the receivers wherein the process was carried on, for which reason I determined to repeat the experiments in receivers of much less diameter.

EXPERIMENT VII.

Inſtead of the laſt-mentioned receiver, Fig. 4, was taken a glaſs tube, of about one inch diameter, and nine inches long,* and furniſhed with lead wire of $\frac{1}{85}$ inch diameter, being the ſame as that uſed in the three firſt experiments. After it had been placed in water, with the cock only above the ſurface and open; and it had remained three minutes, the cock was ſhut, the inſtrument taken out and wiped dry, one end of it was annexed to the chain *b*, of the electrometer, and the other end to the chain which was in communication with the outſide of the battery, the ſame ſtrength of charge was directed through the wire, by the electrometer, as in the laſt experiments, and the ſame kind of powder with reſpect to colour appeared; the inſtrument was again laid in the water, with the cock above the ſurface, and the water gage was ſcrewed on to the cock; when three minutes were expired, the cock was opened, and there appeared ſo extraordinary diminution of air, that the water in the gage run over the upper bend into the inſtrument: this diminution appeared ſufficient to make a mercury gage ſenſible.

The inſtrument being cleaned, and provided with thirty-five inches of wire of the ſame diameter as above, five ſeparate diſcharges were directed through the wire; ſeven inches were exploded at each diſcharge, and the ſame proceſs with reſpect to laying of it in water before and after each diſcharge was obſerved; and the

	Inches.
Firſt diſcharge cauſed ſo extraordinary diminution, that the mercury roſe in the gage	1
Second diſcharge	$1\frac{3}{10}$
Third	$0\frac{5}{10}$
Fourth	$0\frac{5}{10}$
Fifth	$0\frac{3}{10}$
In the whole	$3\frac{6}{10}$

which is nearly equal to two cubic inches. The contents of the whole inſtrument was nine cubic inches.

The air thus diminiſhed in the inſtrument was left to ſtand till the ſame hour next day; but on opening the cock no further diminution had taken place.

The purity of this reſidue of air was tried by the teſt of flame, and it extinguiſhed a candle eight times. Azotic gas, in the ſame glaſs, extinguiſhed the ſame candle ten times, ſo that all the oxigen gas contained in that quantity of atmoſpherical air was nearly abſorbed; the powder produced was nearly ſame as that in the large receiver.

* It was mounted in the ſame manner as Fig. 3, excepting that on account of its diameter only one air cock and gage was uſed.

This

This laſt experiment was repeated with wires of a leſs diameter, with nearly the ſame reſult; the five diſcharges produced a diminution, which cauſed the mercury to riſe between three and four inches.

The reſult of the laſt experiments induced me to try receivers of a ſtill leſs diameter; ſo one of $\frac{4}{10}$ inch diameter, and eight inches long, was taken; but on account of its ſmallneſs of diameter, no more air could be exploded than the length of the tube, without admiſſion of freſh air.

EXPERIMENT X.

Accordingly, one diſcharge of the ſame degree of force was directed by the electrometer through one length of wire $\frac{1}{52}$ inch diameter.

		Inches.
The mercury gage roſe	- - - - - -	$0\frac{3}{10}$
Second diſcharge through wire of $\frac{1}{60}$	- - - -	$3\frac{3}{10}$
Third - - - - - $\frac{1}{85}$	- - - - -	$3\frac{3}{10}$
Fourth - - - - - $\frac{1}{150}$	- - - -	$3\frac{3}{10}$

The wire through which the firſt diſcharge was directed, was only converted into a numberleſs ſmall particles, retaining their metallic luſtre. The ſecond, which ſhewed the greateſt degree of abſorption, was but juſt converted into powder, mixed with particles of lead. The reſidue of this air was tried by flame, in a glaſs five inches high, and one and a quarter diameter, but at the mouth only three quarters, and the candle was extinguiſhed in it nine times.

Theſe experiments, I think, will be found ſufficient to ſhew that the ſmaller the diameter of the receiver in which lead is calcined by an electric diſcharge, the greater proportion of air will be abſorbed, and the ninth and thirteenth experiment proves that, that which is abſorbed is the oxigen gas, which is by the proceſs ſeparated from the azote of the atmoſphere.

It is well known that metals require certain degrees of temperature to give them the property of abſorbing the greateſt quantity of oxigen gas; this is the point which I had in view when I uſed wires of different diameters, becauſe I know no way how to increaſe the temperature communicated by an electric diſcharge to metallic wires, but by diminiſhing theſe diameters or lengths in a certain proportion, or increaſing the force of the diſcharge, which is the ſame thing, and the laſt experiment ſhews that it makes very little difference whether wire of $\frac{1}{60}$, or $\frac{1}{120}$, which is only half that diameter, be uſed, ſo that it does not ſeem here to follow the ſame law as common heat.

EXPERIMENT XI.

To conclude with this metal; the firſt-mentioned large receiver was taken, and 130 inches of wire, of $\frac{1}{85}$ of an inch in diameter, was prepared in the ſame, being nearly four times

times the quantity that was ufed before. This was exploded and converted into an impalpable powder by twenty feparate difcharges, and when the gages were opened, there appeared to have been no air abforbed.

EXPERIMENT XII.

The powder or calx laft produced, was preferved and put into a very fmall glafs retort, with an intent to try what would happen, or what alterations would take place by the application of heat. No air was given out by fuch treatment, excepting a little common air, which I fuppofe was contained in the retort. The powder, which was firft of a blueifh white, changed into a dark leaden colour, and afterwards into a yellowifh green; and when the glafs was red hot, nearly melting the powder, was only partially melted into lead.

Laftly, twenty-four grains of this powder, which had abforbed no oxigen, was taken and put into a clean crucible, with a cover to it, fet in a furnace, and kept in a white heat about one hour. When taken out, there was found in the bottom of the crucible a perfect clear glafs of a redifh brown colour. Thus we find a powder obtained which poffeffes the moft diftinguifhing properties of an oxide of lead, though it appeared to have abforbed no oxigen in its preparation.

Lead burns with a yellowifh flame by the electric difcharge.

Thefe experiments appear to be fufficient to prove, that the abforption of the oxigen gas from the atmofpherical air, by the powder thus produced, wholly depends upon the diameter of the receiver wherein the procefs was carried on. For we find by the tenth experiment, that when eight inches of wire were calcined in a receiver only $\frac{4}{10}$ of an inch diameter, it then abforbed half a cubic inch of air. In this proportion the wire laft calcined fhould have abforbed fixteen times that quantity, and the refult was that it had abforbed none; the reafon of which I muft own I do not underftand.

Thefe experiments were made in the winter feafon, when the temperature of the atmofphere was feldom lower than 28°, and not higher than 45°, and the refult was as mentioned; but not being able to reconcile myfelf to fuch paradoxical refults, I fhowed the experiments to feveral philofophers, who, however, were equally as much at a lofs to account for the refult as myfelf.

On the 10th of May, 1798, I was favoured with the company of fome gentlemen of the Royal Society to fee the experiments. I ufed only two receivers, one of an inch diameter, and the other of fix inches diameter, as follows:

EXPERIMENT XIII.

A piece of leaden wire, of nine inches long, and $\frac{1}{80}$ of an inch in diameter, was exploded by an electric difcharge in the receiver of one inch diameter. After it had laid in water three minutes, the mercury gage rofe $\frac{13}{20}$ of an inch, which was a degree of diminution of about half a cubic inch.

EXPERIMENT

EXPERIMENT XIV.

Eight lengths of wire, of the ſame diameter, was exploded in the receiver of ſix inches diameter; and after it had been in water five minutes, the mercury gage roſe $\frac{2}{10}$ of an inch, which was a degree of diminution of about $3\frac{1}{8}$ cubic inches, and conſequently not agreeing with my former experiments.

The reſult of theſe two experiments occaſioned ſeveral repetitions, and all nearly with the ſame reſult; but on repeating them in the winter, when the temperature was low, I found the reſult as I firſt had (namely) a diminution of air in the ſmall receiver, and none in the large one. It then occurred to me that the temperature muſt have occaſioned this variation, and that putting the receiver in water for five minutes was, perhaps, not ſufficient to bring the air in the inſide of the receiver to its former temperature. I therefore placed a thermometer in the inſide of the large receiver, and by exploding one length of wire, the temperature was raiſed about two degrees; and when the receiver was ſet in water, the air did not return to its former temperature in leſs than twenty minutes; conſequently this pointed out to me one error, which I was not aware of; but it was not ſufficient to explain the whole, becauſe this only would indicate a diminution but leſs than truth. By repeating the experiment at different times, I perceived that the firſt exploſion in a low temperature cauſed the mercury to riſe near two degrees, and that every ſubſequent exploſion affected it leſs. After ſeven or eight exploſions had been given, hardly any riſe in the mercury could be perceived. And alſo when the temperature was at about 60°, the firſt exploſion did not then cauſe the mercury to riſe to above 1°, and it would ceaſe to riſe after the third or fourth exploſion. When the temperature was at about 70°, hardly any riſe in the mercury could be perceived, even at the firſt exploſion; this unfolded the whole, becauſe it appeared that when the temperature was low, the exploſions raiſed it ſo much as to cauſe an expanſion in the incloſed air, which kept up the preſſure upon the mercury in the gages, and of conſequence hindered them from indicating any loſs; and when the temperature was high, the exploſions did not cauſe ſuch expanſion, conſequently permitted the gages to indicate the true loſs, or quantity of air which had been abſorbed by the divided metal.

I thought it proper to mention the above in full, not only for the advantage of ſuch as might undertake ſuch experiments, but likewiſe to prevent wrong concluſions being drawn from the reſult of the firſt ſet of experiments with large receivers, as they have been ſhewn to many, and ſome authors have brought them forward to ſupport their own erroneous notions.

We may alſo conclude from the facts that the difference of diameters of the receivers wherein leaden wire is exploded, neither favours, nor hinders, the diminution of the atmoſpherical air therein contained. If two leaden wires of equal lengths and diameters be exploded by equal electric diſcharges, in unequal receivers, the diminution of the air which

which they contain will be equal in both; and if the receivers contain atmofpherical air, that which is feemingly loft by the procefs is oxigen gas, abforbed by the exploded metal, which becomes converted into a true metallic oxide.

If it be required to collect the produce for further inveftigation, a receiver of the largeft diameter then becomes neceffary, becaufe the oxide is more perfect, and a greater quantity of powder can be produced, and the experiment oftener repeated, before the receiver becomes opaque, which is the cafe when receivers of fmall diameters are ufed.

The foregoing experiments upon lead, prove that a receiver of one inch diameter is fufficiently wide, and therefore preferable to wider, becaufe when the temperature of the air in the infide is raifed by the explofions, it is much fooner brought to its former temperature, and is not fo liable to equivocal refults. I have not, therefore, thought it proper to relate the experiments which were made in wider receivers upon the following metals: fuch receivers being neceffary only when a large quantity of oxide is required.

EXPERIMENT XV.

Tin.

Sixteen inches of tin wire, of $\frac{1}{120}$ part of an inch in diameter, was put into Fig. 4, and after it had been laid in water fo long, that the air in the infide was become of the fame temperature as the water itfelf; it was afterwards exploded by two difcharges, and by that means converted into an impalpable powder, nearly the fame as the lead wire, but of a purer white. The apparatus was then again laid in water * till the air had gained its former temperature; when the cock was opened, the mercury rofe three inches, and the air, by the teft of flame, feemed to be as as highly azotic as that in which lead had been exploded in the ninth experiment.

The laft experiment, repeated with wire of $\frac{1}{100}$ of an inch diameter, and the refult was the fame.

Tin burns with a vivid yellow flame when exploded.

EXPERIMENT XVI.

Iron.

Thirty-two inches of iron wire, of $\frac{1}{230}$ inch diameter, was put into Fig. 4, and exploded by five difcharges: the mercury rofe three inches.

The wire was converted into an impalpable powder, of a reddifh brown colour; the refidue of air feemed to be the fame as that left in the ninth and fifteenth experiments, wherein lead and tin had been exploded. It extinguifhed a candle nine times.

* The apparatus was always laid in water a fufficient time to caufe the air in the infide to become of the fame temperature as the water, before and after the metal was exploded; but to avoid prolixity I have not mentioned it in the relation of the following experiments.

EXPERIMENT XVII.

Wires of different diameters were taken, with a view to try what degree of fusion was required to cause the greatest absorption of air, or, in other words, to make the most complete oxide.

The same degree of strength of charge was directed through wire of $\frac{1}{100}$ inch diameter; no air absorbed; the wire was only red hot.

Wire of $\frac{1}{100}$	was exploded and converted into balls; the mercury rose	$0\frac{6}{10}$
—— $\frac{1}{130}$	converted into very small powdered balls - -	$0\frac{6}{10}$
—— $\frac{1}{150}$	into flocculi and balls - - - -	1
—— $\frac{1}{240}$	into an impalpable powder of reddish brown colour -	$1\frac{5}{10}$

EXPERIMENT XVIII.

Copper.

Thirty-two inches of copper wire, of one 260 part of an inch diameter, was prepared in Fig. 4, and exploded by five discharges; the mercury in the gage rose $1\frac{1}{10}$ inches.

It was converted into an impalpable powder, of a purple brown colour; the residue of air extinguished the flame of a small wax taper twice.

This metal burns with a green flame when exploded.

All the foregoing experiments were made by myself, at my house in Poland Street; but the following were made at the house of Mr. Campbell, in South Audley Street, who honoured me with his assistance. The apparatus we used were of my own construction, being a double plate machine; each plate twenty-six inches diameter, and a double battery of thirty jars of my common size, containing about thirty-four square feet coated glass. The electrometer was the same as I have described in Nicholson's Journal, Vol. II. page 528, excepting that instead of using pins of certain weights to put into B, the arm C B was divided into grains, with a slider upon it, which was set to the weight required, as I commonly make them at present; being more convenient.

EXPERIMENT XIX.

Pure Silver.

Forty-five inches of silver wire, of $\frac{1}{130}$ part of an inch diameter, was exploded by five discharges; the slider upon the electrometer was set at forty grains; it was converted into an impalpable black powder: the mercury rose in the gage about half an inch, and the flame of the taper was extinguished nine times.

Silver burns with a greenish flame, nearly resembling that of copper.

 EXPERIMENT

EXPERIMENT XX.

Grain Gold.

Fifty-four inches of wire drawn from grain gold, of $\frac{1}{125}$ part of an inch diameter, was exploded by six discharges; the slider upon the electrometer was set at forty-two grains; it was converted into an an impalpable powder, of a dark brown purple colour: the mercury rose in the gage about a quarter of an inch, and the flame of a taper was extinguished four times.

EXPERIMENT XXI.

Nine inches of the same sort of wire, but of a less diameter, being 130 part of an inch, was exploded in the same receiver by one discharge: the mercury rose about $\frac{2}{10}$ of an inch, and the flame of the taper was extinguished four times.

EXPERIMENT XXII.

Platina.

Twenty-seven inches of platina wire, of 130 part of an inch diameter, was exploded by three discharges. When the gage cock was opened, the mercury rose a quarter of an inch, but on applying the flame to the residue, it was not extinguished.

As platina is easily fused by electricity, the slider upon the electrometer was only set at twenty-two grains for the last experiment, and the residue of air did not extinguish the flame. It was supposed that too low a charge had been used. The slider was therefore set at thirty-two grains, and the experiment was repeated with the same length and diameter of wire, and exploded by the same number of discharges; when the cock was opened, the mercury rose about one inch, and the flame of the taper was extinguished five times: the metal was converted into a black impalpable powder, resembling that of silver.

EXPERIMENT XXIII.

Zinc.

Nine inches of zinc wire, of $\frac{1}{100}$ part of an inch in diameter, was exploded by one discharge; when the gage cock was opened, the mercury rose half an inch, and the residue of air extinguished the flame of a taper twice: the metal was converted into an impalble powder of a white colour, resembling that of tin.

This metal being so difficult to draw into wire, I was obliged to conclude with the above-mentioned length.

From the result of the foregoing experiments, it may be safely concluded, that all the ductile metals can, by electric discharges, be sublimed and converted into proper oxides, by absorbing the oxigen of the atmosphere; and though some of the metals resist the action of common fire, and require different solvents to convert them into oxides, yet they all yield to the action of the electric fluid.

4 It

It is remarkable that platina, though it refifts the action of common fire, is more eafily fufed by electric difcharges than copper, filver, or gold, and feems to be as greedy of oxigen as any of the other metals. But thefe experiments have not been fufficiently extended to fettle the laft mentioned property.

It is well known that all metals which are fublimable by common fire, abforb oxigen in different degrees, and likewife in different proportions, according to the different degrees of heat employed; this feems likewife to take place when they are fublimed by electric difcharges, but the proper degree of difcharge for each metal remains for inveftigation; and as different metals differ as conductors of the electric fluid, (which has never yet been well defined) this furnifhes matter fufficient for a future purfuit.

The lateft experiments upon the conducting power of metals were made by Dr. Van Marum and myfelf, at Haerlem, but as I had not at that time invented the electrometer which I have at prefent, and which the nature of the experiment required, I have fome reafon to think that they are not perfectly accurate. I have concluded thefe experiments, without proceeding to the nonductile metals, becaufe I know no way of arranging the others, fo that they can be acted upon with fufficient accuracy.

The experiments which have been made upon the different metals mentioned, have been very numerous, excepting zinc, but for the fake of brevity, and to avoid tautology, I have only mentioned thofe which I thought to be abfolutely neceffary.

V.

Defcription of an Improvement in the Steam Engine, by means of which the Boiler is conftantly fupplied with Water already heated nearly to the point of Ebullition. Communicated by Mr. PETER KEIR, *Engineer.**

THE fteam engine, of which we have fo many accounts in introductory and other works, and which for its fcientific contrivance and extenfive utility cannot be too highly regarded, may be traced in its hiftory through a few great and ftriking degrees of improvement, the intention of every one of which confifts in producing the greateft quantity of moving force with the leaft poffible confumption of fuel. It is probable that the original engine of the Marquis of Worcefter may have been a mere veffel into which water flowed by its weight, and was thence driven upwards by fteam: Capt. Savary feems to have been the firft who ufed the vacuum formed by fteam; into which the water was urged by the atmofphere: Papin in a crude way, and Newcomen and Cawley in a more perfect method, ufed a folid pifton: and laftly, James Watt greatly improved the engine of the latter ope-

* In converfation. I am very forry that the latenefs of the date of this fheet at prefs, does not allow me time to fend and inquire the name of the inventor, which I omitted to write down. The improvement is under patent, which, with the name, I fhall mention in my next.

rators by condenſing his ſteam in a cold veſſel at a diſtance from the working cylinder. The other improvements in the mechaniſm in the furnace, in the double ſtroke, &c. &c, though highly valuable and honourable to the ſkill and contrivance of the inventors, appear to be of ſubordinate importance, and in many particulars are little more than thoſe variations of faſhion which in mechanics, as well as other leſs important departments of human induſtry, are more or leſs prevalent.

It was among the early improvements of the ſteam engine, to feed the boiler out of that hot water which was afforded by the condenſation of the ſteam along with the ſtream of injection. When the boiler receives a quantity of cold water, it becomes neceſſary either to increaſe the fire, or to admit the engine to work with leſs effect from the diminiſhed quantity or elaſticity of the ſteam produced; and when the water thus ſupplied is afforded in the manner juſt mentioned, it will be hotter the leſs the injection, and the contrary. But there is a mean which muſt be attended to in the adjuſtment of this heat. A ſparing injection will not afford a good vacuum, and therefore the engine will work with diminiſhed effect; and a copious injection will afford water at a low temperature to the boiler. When the adjuſtment is at the beſt, the water of the ſupply is not hotter than the hand can bear, that is to ſay, about 120°. It becomes therefore an object of value, to aſcertain the means by which the condenſation of the ſteam may be rendered as perfect as the introduction of cold water can make it, and at the ſame time, that a greater part of the heat it imparts may be employed in raiſing the temperature of the water intended for the boiler.

Plate VIII. repreſents a ſection of the parts of a double engine; that is to ſay, an engine in which the piſton is alternately impelled by the elaſticity of ſteam acting above and below. A A is the great cylinder in which the piſton moves; O, P, are the ſteam boxes, or metallic receptacles, into which the ſteam is firſt introduced from the boiler by pipes at O and P, not repreſented in the figure. They contain each two valves, B, C, and *b*, *c*, which are opened or ſhut by the uſual apparatus attached to the working beam; *d*, *d* is the paſſage to D D the condenſer, terminating in H the air pump. E F G are the additional parts of which the new improvement conſiſts.

Let us then ſuppoſe the engine in actual work, the valve *b* in the lower ſteam box being open, and *c* ſhut, while B in the upper ſteam box is ſhut, and C open. From this diſpoſition ſteam will ruſh in from the boiler through *l*, beneath the piſton, while the other ſteam which had been admitted before above the piſton, will have a communication through L, C, K and *d*, to D the condenſer, where a conſtant injection J being kept up, it becomes condenſed and leaves a vacuum. Conſequently the piſton muſt riſe by the whole force of the ſteam beneath. When it has arrived near the top of the cylinder, the valves in the ſteam boxes *b* and C are ſhut, and the others B and *c* are opened; ſo that the ſteam from the boiler ruſhes through L into the ſpace above the piſton, while the other ſteam beneath paſſes through *l*, *c* and *k* to the condenſer, where it aſſumes the denſe form and leaves a vacuum, into which the piſton is urged in its deſcent. When the deſcent is completed, the

the valves refume their firft ftate, and the pifton muft again rife, &c. &c. The pump H is kept conftantly at work by the motion of the engine, and carries off the water of the injection heated by the fteam, and alfo the permanently elaftic fluid, which may be extricated from the water: and of this hot water it is that the boiler in fteam engines is ufually replenifhed. Inftead however of this direct fupply in the prefent engine, the hot water is fuffered to flow into the fmaller refervoir G, G, G, which it keeps conftantly full and overflowing, and from which a portion is raifed by the fmall pump E through the pipe F, M, F, F, tranfmitted through the paffage to the condenfer, and confequently having its exterior furface in contact with the hot fteam as it rufhes towards the condenfer, and by that means having its contents, and confequently thofe of the veffel G G G, greatly heated by the circulation effected by the pump. The boiler is fupplied by the pipe N, placed fo far below the return M, that the preffure of the column I M may exceed the re-action of the fteam in the boiler, which might otherwife prevent its introduction.

I am not prepared to fay to what extent this improvement may add to the power of the engine.

VI.

Inquiries relative to the Laws of Affinity. By Cit. Berthollet.*

(*Continued from page* 108.)

(*Continued from page* 108.)

On the Influence of the Proportions in Complex Affinities.

1. IN my former inquiries I have afcertained the principal refults afforded by complex affinity, when the force of cohefion, or that of elafticity are fufficiently great to produce that change of bafe which had been attributed to the fuperiority of the divellent affinities over the quiefcent affinities. But I have neglected to enter into the neceffary details for determining the changes which may arife from different proportions of the fubftances which are brought into action, when the force of cohefion is not fufficiently great to caufe the effects of this difference to difappear. I have promifed (in Art. XII. No. 6,) to return to this fubject, which I fhall now proceed to do.

According to the theory which I have laid down (Art. V. No. 5), all fubftances exert a mutual action during the time they are in the liquid ftate; fo that in a folution, for example, of fulphate of pot-afh and muriate of foda, thefe two falts are not diftinct, while there is no caufe to determine the feparation from their combination; but that there exifts in this liquid, fulphuric acid, muriatic acid, foda, and pot-afh. I fhall, however, continue to make ufe of the ordinary language, which after this notice, ought not to produce any ambiguity.

* Annales de Chimie, XXXVIII, 3.

2. I begin with mixtures in which a confiderable force of cryftallization muft determine the combinations which are formed.

Experiment A. Equal parts of nitrate of lime and fulphate of pot-afh were mixed: after the feparation of the fulphate of lime which was firft formed, and of which I fhall fay more in the following experiments, the liquid was evaporated, and nothing was obtained by the fucceffive operations but nitrate of potafh and fulphate of lime. After the laft evaporation, however, fome cryftals of fulphate of pot-afh were procured; there remained but a very fmall quantity of uncryftallizable liquid, which precipitated with the carbonate of foda and with the nitrate of barites; fo that it confifted of a little fulphuric acid and lime, and very probably a larger portion of nitrate of pot-afh.

The quantity of fulphate of lime which was depofited in the courfe of the evaporation, was much more confiderable than would have been obtained from fimple folution of this falt in water; fo that its folubility was augmented by the action of the other fubftances.

Experiment B. Two parts of fulphate of pot-afh, and one of nitrate of lime, gave by a firft evaporation fulphate of potafh and fulphate of lime; and by the fucceeding evaporations, nitrate of pot-afh with the two fulphates, the proportions of which continued to decreafe to the laft cryftallization. There remained only fome drops of the liquid uncryftallized; they did not precipitate with the carbonate of foda, but did with the nitrate of barites: hence they were probably formed of fulphate of pot-afh, and a fmall proportion of nitrate of pot-afh.

Experiment C. Two parts of nitrate of lime, and one part of fulphate of pot-afh, afforded during the firft evaporation a fmall quantity of fulphate of lime, and by cooling, nitrate of pot-afh; the other evaporations only produced nitrate of pot-afh. In the latter, however, fome cryftals of fulphate of lime were perceived at the furface of the liquid. Notwithftanding the refidue, which was abundant, was feveral times fubmitted to evaporation and cooling, it no longer afforded cryftals of any falt. This uncryftallizable refidue treated with alcohol, formed an abundant depofition, which having been diffolved in water, fcarcely afforded any precipitate with the nitrate of barites; fo that it contained little or no fulphuric acid, and confifted of pure nitrate of pot-afh: the portion diffolved by alcohol was nitrate of lime, with a fmall portion of nitrate of pot-afh. The uncryftallizable refidue was therefore compofed of nitrate of pot-afh and nitrate of lime.

In this experiment we fee, that the fulphate of lime was rendered much lefs foluble than in the preceding experiments; but that a confiderable quantity of nitrate of pot-afh loft the property of cryftallizing by the action exerted on it by the nitrate of lime.

3. In thefe three experiments fulphate of lime muft have been formed, becaufe the lime and fulphuric acid coming in contact, would feparate on acccount of the infolubility which belongs to their combination.

The fulphate of lime in the experiment A and in the experiment B, was rendered much more foluble than it naturally is, by the action of the fubftances which were in folution; but in the experiment C, its folubility was not fenfibly increafed, probably becaufe the nitrate

nitrate of lime and the nitrate of pot-ash which formed the uncryftallizable liquid, mutually experienced a degree of faturation, which confiderably weakened their action upon the fulphate of lime.

4. From thefe confiderations, I fhall proceed to deduce firft, the theory of the uncryftallizable refidues which are found in the folutions of falts which are evaporated: it will be confirmed by the fucceeding obfervations.

Saline fubftances exercife upon each other an action which increafes their folubility, an effect that has been particularly eftablifhed by the experiments publifhed by my learned colleague Vauquelin (*Annales de Chimie*, tom. xiii.) This mutual action varies in the different falts. It has however been thought, that falts with an earthy bafe do not increafe the folubility of the nitrate of pot-afh, though thefe in reality increafe it the moft.

There is doubtlefs in this refpect, a difference in the effect produced by the falts, which depends on their nature; but this difference is in general very fmall, compared with that which proceeds from the force of cryftallization.

Experiment D. A mixture of equal parts of nitrate of pot-afh and fulphate of pot-afh, afforded by evaporation, fucceffively and in proportion to their folubility, fulphate of pot-afh and nitrate of pot-afh, without leaving any uncryftallizable liquid; but the fame experiment being made with a mixture of nitrate of foda and fulphate of foda, both of which have only a flight tendency to cryftallize, and are nearly of equal folubility, only a fmall quantity of fulphate of foda is feparated by cryftallization, all the reft remaining liquid, without any cryftallization. A mixture of muriate of foda and fulphate of alumine having been fubmitted to the fame proof, it was clearly perceived that the two falts had become more foluble; but they were intirely feparated by the alternate evaporation and cooling.

It is therefore feen that fubftances which poffefs a confiderable force of cryftallization, though rendered more foluble, feparate on account of their infolubility, and leave very little or no uncryftallizable refiduum.

But when falts are found which have only a feeble difpofition to cryftallize, their mutual action counterbalances their force of cryftallization, fo that there then remains much liquid which cannot cryftallize; and this happens particularly when the refiduum contains a fubftance in itfelf uncryftallizable; as in experiment C, where by the proportions employed, there was found a fuperabundance of nitrate of lime, which by its action upon the nitrate of pot-afh reduced a confiderable quantity of that falt into an uncryftallizable liquid.

Whence therefore does it happen, that the moft learned chemifts, Lavoifier, Fourcroy, Vauquelin, Guyton, and the Commiffaries of the Academy of Sciences, among whom myfelf was one, could have been led by the experiments made upon the proof of falt-petre, to believe, that the nitrate of lime exerts no action upon the nitrate of pot-afh, nor increafed its folubility? (*Ann. de Chimie*, tom. xi. xiii. xv. xxiii.) It was becaufe, in the experiments that were made, a water of nitrate of pot afh was put to digeft upon dried nitrate of lime. The latter neceffarily caufed a divifion of the water upon which it has a ftrong

ſtrong action. It would therefore have precipitated a conſiderable quantity of nitrate of pot-aſh, if this effect had not been compenſated, or nearly ſo by the ſolubility which it imparted to that ſalt; but if the ſolution had been evaporated, there would have been extracted by cryſtallization much leſs nitrate of pot-aſh than was contained in the liquid, and an uncryſtallizable reſiduum would have been left ſimilar to that of experiment C. The proof therefore which might be conſidered as concluſive for the object propoſed, has induced an error reſpecting the chemical phenomenon.

Hence it follows, that when the mother waters of the ſalt-petre works are decompoſed by pot-aſh, the nitrate of pot-aſh obtained conſiſts not only of that which has juſt been formed, but alſo that which was rendered uncryſtallizable by the ſalts with an earthy baſis.

5. I made mixtures of cryſtallized ſulphate of ſoda and nitrate of lime. Here the ſulphate of ſoda, and the nitrate of ſoda which might proceed from the operation, differed little in point of ſolubility, and had a cryſtallizing power leſs conſiderable than the ſulphate and nitrate of pot-aſh.

Experiment E. Equal parts of cryſtallized ſulphate of ſoda and nitrate of lime, reduced to the ſtate of dryneſs, gave by evaporation only a ſmall quantity of nitrate of ſoda. The uncryſtallizable reſidue, which was abundant, did not precipitate with the muriate of barites, but with the oxalic acid.

Experiment F. Two parts of ſulphate of ſoda, and one part of nitrate of lime, afforded after evaporation, a greater quantity of nitrate of ſoda than the preceding experiment; the uncryſtallizable reſidue did not precipitate with the oxalic acid, but with the muriate of barites. In order to compare the two laſt experiments with the experiments A, B and C, it muſt be obſerved, that the cryſtallized ſulphate of ſoda contains more than half its weight of water of cryſtallization.

Even in experiment F no ſulphate of ſoda is formed, as ſulphate of pot-aſh was formed in experiments A and B, becauſe the ſulphate of pot-aſh has a force of cryſtallization much greater than the ſulphate of ſoda.

The reſidue of experiment E did not contain ſuch a quantity of ſulphuric acid as could be rendered perceptible by the muriate of barites; but it was formed of nitrate of ſoda and nitrate of lime, which probably, by their mutual action, ſo much exhauſted their diſſolvent power, as not to prevent the ſeparation of the ſulphate of lime No. 3.

In experiment F the reſidue was not precipitated by the oxalic acid, but by the muriate of barites; ſo that this reſiduum was compoſed of ſulphate of ſoda, and a greater proportion of nitrate of ſoda, which mutually prevented their cryſtallization, as in experiment D.

6. *Experiment* G. Equal parts of nitrate of pot-aſh and ſulphate of ſoda, gave by ſucceſſive cryſtallizations, 1. ſulphate of pot-aſh, and ſome ſmall cryſtals of nitrate of pot-aſh; 2. a little ſulphate of pot-aſh, and a greater proportion of nitrate of pot-aſh; 3. ſmall cryſtals of nitrate of pot-aſh, and much nitrate of ſoda: there was an uncryſtallizable reſidue, notwithſtanding

notwithſtanding the care which had been taken to obtain the utmoſt cryſtallization; this reſidue was formed of nitrates and ſulphates; for it precipitated abundantly with the nitrate of barites, and after drying it flowed on ignited charcoal. This experiment ought to be compared with experiment F.

Experiment H. One part of nitrate of pot-aſh, and two of ſulphate of ſoda, gave, 1. ſulphate of pot-aſh; 2. ſulphate of pot-aſh, and ſome needles of nitrate of pot-aſh; 3. ſulphate of pot-aſh in ſmall priſms, ſuſpended from a pellicle formed by nitrate of ſoda, fine cryſtals of nitrate of pot-aſh and nitrate of ſoda; the reſidue contained nitrates and ſulphates.

In theſe two experiments, the leaſt ſoluble ſalt of thoſe which were formed, *viz.* the ſulphate of pot-aſh, was the firſt which cryſtallized. When the proportions had been thus changed, the action of the nitric acid on the pot-aſh reſumed its ſuperiority, and nitrate of pot-aſh was formed, though the liquid ſtill contained ſulphuric acid.

In the ſecond, the more abundant ſulphuric acid gave riſe to a greater quantity of ſulphate of pot-aſh; but after the firſt cryſtallization, more nitrate of pot-aſh was formed, though there would have been a ſufficient quantity of ſulphuric acid to cauſe a complete change of baſe, if this exchange could have been made as has been imagined. The reſidue was formed even in experiment G, of ſulphate of ſoda and nitrate of ſoda; and probably of a ſmall quantity of ſalt with baſe of pot-aſh.

7. *Experiment* I. Equal weights of nitrate of pot-aſh and muriate of lime were mixed together: by evaporation there were obtained, 1. nitrate of pot-aſh; 2. muriate of pot-aſh, in which was found a little nitrate of pot-aſh: the reſidue gave with the ſulphuric acid an abundant precipitate of ſulphate of lime, and vapours of muriatic and nitric acid were diſengaged.

Experiment K. The experiment having been made with two parts of muriate of lime, and one of nitrate of pot-aſh, an abundant cryſtallization was effected of muriate of pot-aſh with no appearance of nitrate; the reſidue treated as in the preceding experiment, gave analogous reſults.

Experiment L. A mixture was made the reverſe of thoſe of the preceding experiments; it was formed of equal parts of muriate of pot-aſh and nitrate of lime. It afforded, 1. nitrate of pot-aſh, mixed with a little muriate of pot-aſh; 2. muriate of pot-aſh, which was mixed with a ſmall quantity of nitrate of pot-aſh. The uncryſtallizable reſiduum was diſſolved in alcohol; there was a ſeparation of nitrate of pot-aſh, which was fuſed upon ignited charcoal, but contained a little muriate of pot-aſh, as was ſeen by the teſt of the ſolution of ſilver. The ſulphuric acid ſhewed, that the part diſſolved by the alcohol contained muriatic acid, nitric acid, and lime.

In the experiments above deſcribed, and in which ſubſtances were employed whoſe combinations could not have a conſiderable force of cryſtallization, and differed but little among each other in this reſpect, it is evident, that the formation of the ſalts which were afterwards obtained by cryſtallization, depended on the proportions of the ſubſtances which mutually acted among each other. In experiment I, which might afford nitrate and muriate of pot-aſh; as theſe two ſalts differed little in their ſolubility, which however is rather the

the leaſt in the latter, the nitrate of pot-aſh was obtained by the firſt cryſtallization; but as in experiment K the muriatic acid exiſted in a greater proportion, muriate of pot-aſh only was then obtained: one part of pot-aſh with muriatic acid, nitric acid and lime formed the reſiduum. The proportions employed in experiment L, differed little from thoſe of I, and the reſults were nearly the ſame.

Oppoſite combinations therefore are obtained according to the proportions employed, or according to the period of cryſtallization; that is to ſay, according to the proportions of the ſubſtances which remain in action, when a ſufficient force of coheſion does not exiſt in the combinations that might be formed. But according to the opinion adopted by chemiſts, the entire change of baſe which it is affirmed muſt take place, may be judged of even by a firſt cryſtallization.

If, for example, nitrate of pot-aſh be firſt obtained from the proportions which have been employed of muriate of pot-aſh and nitrate of lime, a concluſion is made, that an exchange of baſe has been effected between the muriatic and nitric acids. If other proportions had been employed which would have afforded muriate of pot-aſh, as in experiment K, an oppoſite conſequence would have been drawn. Chemiſts have even gone farther: from the concluſion reſpecting the change of baſe, for example, between the muriatic and nitric acids, a farther inference has been made, that the combinations oppoſite to thoſe which afforded ſuch a change, would not be at all affected by their mixture.

8. I have examined what change might be produced in the reſults by a ſubſtance which has the property of forming triple ſalts, ſuch as magneſia.

Experiment M. Equal parts of ſulphate of pot-aſh and muriate of magneſia, afforded, 1. ſulphate of pot-aſh; 2. ſulphate of pot-aſh, a little muriate of pot-aſh, and a triple ſalt, compoſed of ſulphuric acid, pot-aſh, and magneſia; this ſalt forms fine rhomboids, which, by expoſure to the air, do not loſe their tranſparency; its ſolubility is nearly the ſame as that of the ſulphate of pot-aſh: 3. muriate of pot-aſh and ſulphate of magneſia. The reſiduum contained ſulphuric acid, muriatic acid, pot-aſh, and magneſia.

Experiment N. Two parts of muriate of magneſia, and one of ſulphate of pot-aſh, afforded, 1. ſulphate of pot-aſh; 2. muriate of pot-aſh, and the triple ſalt of the preceding experiment; 3. muriate of pot-aſh, and ſulphate of magneſia: the reſiduum was analogous to that of the preceding experiment.

In experiment M, ſulphate of pot-aſh was obtained by two cryſtallizations; but in experiment N, where the muriatic acid was in a greater proportion, it was obtained on the firſt cryſtallization; the triple ſalt, which in ſolubility nearly approaches the ſulphate of pot-aſh, cryſtallized after the ſecond evaporation. When the proportion of ſulphuric acid was ſufficiently diminiſhed by theſe cryſtallizations, then the muriate of pot-aſh was ſeparated, nearly according to the order of ſolubility; and laſtly the magneſia, which being ſtill abundant, cryſtallizes with part of the ſulphuric acid.

We ſee that a different opinion might have been formed in theſe two experiments, according to the period of the cryſtallization to which the attention might have been directed,

 and

and how far diſtant from truth the eſtabliſhed opinion is, that a complete change of baſes takes place on the mixture of muriate of magneſia and the ſulphate of pot-aſh.

In experiment M, where the ſulphuric acid was preſent in greater proportion, ſulphate of pot-aſh was obtained in both the firſt cryſtallizations; but in experiment N, where the muriatic acid had more influence by its relative quantity, ſulphate of pot-aſh was obtained only in the firſt cryſtallization; the magneſia, for the moſt part, remained in the uncryſtallizable reſidue, becauſe it has no force of cryſtallization with the muriatic acid, and only a weak power with the ſulphuric acid; other ſalts which would in other circumſtances have been cryſtallizable, are retained in the reſidue which oppoſes their cryſtallization.

The difference obſerved between the reſults of theſe two experiments, and thoſe of the experiments A, B, and C, in which a deliqueſcent calcareous ſalt was put into action with the ſulphate of pot-aſh, correſponds exactly with the difference of ſolubility which exiſts between the ſulphate of lime and the ſulphate of magneſia.

9. After having treated of ſaline ſubſtances in the preceding experiments, as if they formed ſeparate or diſtinct combinations in a liquid, I ſhall now conſider ſome effects which are owing to the mutual action which is in reality exerted by them all when they are mixed in this ſtate.

Experiment O. Aqueous ſolution of the acetite of lead was gradually poured into a ſolution of muriate of ſoda, till no more precipitate was produced. The liquid which remained above the precipitate ſtrongly reddened blue paper, which effect was not produced by the mere ſolution of the acetite of lead, nor by the muriate of ſoda; but it aſſumed a deep colour with the hydro-ſulphurets, and afforded an abundant precipitate with the muriatic and ſulphuric acids. During its evaporation a depoſition was formed of a ſcarcely ſoluble muriate of lead, and alſo a cruſt which had not a cryſtalline appearance, and which will be ſpoken of hereafter. Laſtly, ſome fine cryſtals were obtained, which were an acetite of ſoda, and an oxide of lead. On diſſolving the ſaline cruſt, a depoſition took place of muriate of lead with exceſs of oxide, analogous to that deſcribed by Vauquelin (*Annales de Chimie*, t. xxxi.) It was neceſſary to repeat the ſolution and cryſtallization ſeveral times, before the depoſition ceaſed. The ſaline cruſt was thus, or at this period, divided into two ſubſtances, muriate of ſoda, and muriate of lead.

If the muriatic acid remain engaged in the acid together with the acetous, as both acids are volatile, the exceſs which is but ſlightly engaged, and which may be expelled by the action of heat, muſt be compoſed of two acids. In fact, having diſtilled a mixture ſimilar to the preceding, after having ſeparated the depoſition which was firſt formed, the liquid which paſſed into the receiver contained acetous and muriatic acid; this circumſtance deſerves particular attention, and ſerves to explain ſeveral obſervations.

10. Citizen Prieur has obſerved that when lead is employed for purifying the ſolution of ſilver mixed with muriatic acid, a part of the laſt-mentioned acid paſſed over by diſtillation. This happens becauſe the muriate of lead is ſoluble, and even very much ſo by the action of the nitric acid. The liquid is therefore compoſed of oxide of lead, muriatic acid

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and nitric acid. The oxide of lead divides its action upon the two acids, and both are subjected to the action of expansibility produced by heat.

If the sulphuric acid were to be retained, lead would be an efficacious medium; 1. because the sulphate of lead is much less soluble than the muriate; 2. because the sulphuric acid is much less volatile than the muriatic acid.

The muriate of silver being much less soluble than the muriate of lead, silver is much better than lead for retaining the muriatic acid which is found mixed with nitric acid. Velter and Bonjour have nevertheless observed that muriatic acid always came over in distillation, when the operation was made without the precautions pointed out by those learned chemists. To obtain a pure nitric acid immediately, it is necessary to make the operation upon an acid little concentrated, in order that it may not hold in solution any muriate of silver, and to separate the muriate of silver which precipitates before the liquid is subjected to the action of heat, or which is preferable, to precipitate by the solution of silver, the muriatic acid from the nitrate of pot-ash; after which, by decomposing this nitrate, the nitric acid is obtained perfectly free from muriatic acid.

When muriate of silver remains in the solution, a very pure nitric acid may nevertheless be obtained by distillation, by setting aside the first portion which distils over, till it is seen by the test that no more muriatic acid is afforded. For as the chemists I have just quoted observed, the muriatic acid in this process assumes the nature of oxigenated muriatic acid, and is disengaged in this state at the beginning of the operation.

Experiment P. Sulphate of pot-ash having been treated with the acetite of lead, in the same manner as the muriate of pot-ash, sulphate of lead was precipitated. The liquid retained only a small quantity of oxide of lead. By the progress of evaporation some crystals of sulphate of pot-ash were obtained, though before the evaporation the acetite of lead produced no more precipitate; and, lastly, the acetite of pot-ash, which retained a small quantity of oxide of lead. The decomposition of the sulphate of pot-ash was much more complete than that of the muriate of soda.

In the experiments I have just described, we see, then, that the decompositions, or exchange of bases, also follow the order of the solubilities of the combinations which may be formed, and that the only difference observed in most of the preceding experiments arises from the property possessed by the oxide of lead of forming triple combinations, which sometimes again separate in combinations of different degrees of solubility, as happens with the muriate of soda and of lead. Experiment O.

11. The following experiment will confirm the truth that the force of cohesion produces a different effect, according to the properties of the solvent.

Experiment Q. I mixed an aqueous solution of plombate of soda with the water of sulphate of soda, a slight precipitate only was produced, though the sulphuric and muriatic acids, if added, would have produced an abundant precipitate: when the experiment was made with the muriate of soda, the precipitate was much more abundant than with the sulphate of soda.

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On the firſt view, theſe effects may appear to be contrary to the principles I have eſtabliſhed. For the ſulphate of lead is much leſs ſoluble than the muriate of lead; ſo that it might be expected that a more abundant precipitate would be obtained in the experiment made with the ſulphate of ſoda than in that with the muriate of ſoda. This happens otherwiſe becauſe the ſulphate of lead is much more ſoluble in ſoda, as I have aſcertained, than the muriate of lead, with exceſs of oxide, ſuch as is precipitated in the preceding experiments. And the precipitation is not a direct conſequence of the force of coheſion, but ariſes from the exceſs of the force of coheſion beyond that of the ſolvent.

12. The obſervations which I have preſented in this memoir may be reduced to the following reſults. In the complex affinities, or double affinities, the force of coheſion, when conſiderable, and differing much in its intenſity among the combinations which may be formed, determines a change of baſes, in ſuch a manner that the moſt inſoluble combination is formed and ſeparated independently of the proportions, which have an influence only upon the ſtate of thoſe ſubſtances which remain in ſolution. The reſult of a mixture of different ſaline ſubſtances may therefore be foretold from the mere conſideration of ſolubility.

In this caſe the adopted theory of the quieſcent and divellent affinities does not miſlead us, as to the principal reſult, that is to ſay, the formation of the inſoluble ſalt. But as it is not deduced from facts of a ſuperior order, it will demand as many experiments as particular facts. The doctrine is not eſtabliſhed upon any foundation from which we can foretel the mutual actions of ſubſtances preſented to each other; and again it has the diſadvantage of affording no indication reſpecting the properties which the remaining fluid portion ought to exhibit when ſubjected to evaporation, or to the action of a new ſubſtance.

Though this certain relation in the reſults may be productive of doubt as to the theory in the foregoing caſe, it is not the ſame when there is but a ſmall diſtance between the ſolubility of the combinations that may be formed. The proportions of the ſubſtances, in quantity, then determine the formation of the different ſalts, either by a firſt cryſtallization, or by cryſtallizations which, by ſubtracting ſome of the parts, muſt alter the proportion, and occaſion a difference in the diſpoſition of the remaining principles to cryſtallize in the ſucceſſive formation of the ſalts. It is here that the application of the theory of the quieſcent and divellent affinities may produce many errors, by leading us to conclude from the commencement of the phenomenon that the ſubſequent effects will be all of the ſame deſcription, though in reality a ſucceſſion of oppoſite combinations may be eſtabliſhed, according to the forces which are made to act at the moment of their reſpective ſeparations.

The joint conſideration of the difference of ſolubility, and of the proportions employed, or which vary at the different periods of an operation, muſt conſequently be our ſole guide in the explanation of the ſucceſſive formation of different ſalts which have no great difference in ſolubility. It is true, nevertheleſs, that the mutual action of the ſubſtances themſelves may produce ſome difference in the reſults, which would be indicated by the preceding obſervations.

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All ſubſtances which are in ſolution exert a mutual action, which increaſes their ſolubility. Hence the reaſon why it is difficult to obtain, by a firſt cryſtallization, each ſalt in a ſtate of purity; except in the caſe where it differs conſiderably from the others in its force of cryſtallization. Hence ariſe the uncryſtallizable reſidues which ſucceed the cryſtallizations wherein ſalts are found in the liquid, which poſſeſs but little force of coheſion. But here alſo the conſideration of the proportions and of the ſolubility will ſtill be uſeful to predict the exiſtence and compoſition of an uncryſtallizable reſiduum.

While the ſubſtances are in ſolution, the action which they mutually exert renders it eaſy to expel an acid from a combination, though, according to the received opinion, it ought to aſſume the place of another, and which was ſuppoſed to be weaker.

When inſolubility is conſidered, it muſt not be regarded as an abſolute property, but as a property relative to the liquid in which a precipitation is made. Thus a combination inſoluble in water may loſe this property when the water holds alkali in ſolution.

In all the experiments I have deſcribed, and in ſeveral others which I have thought it uſeleſs to deſcribe, I perceived no change of ſaturation, either after the mixture of the neutral ſalts, or after the ſeparation of the precipitates or cryſtallizations which took place, except in the experiments P, Q, made with a metallic ſubſtance. This permanent ſtate of neutralization, after the change of baſes which took place, ſeems to indicate that the acids have conſtant relations of quantity, in the neutral ſalts they form, with different alkaline or earthy baſes. So that if the ſulphuric acid for example, be found in greater proportion in the ſulphate of pot-aſh than in the ſulphate of lime; the muriatic acid with which it may make an exchange of baſe will be found in the ſame ratio of quantity in the muriate of lime and in the muriate of pot-aſh; a concluſion that would not agree with the proportions which have often been attributed by chemiſts to the component parts of the different non-metallic ſalts. Guyton has already made ſeveral very juſt and important reflexions on this ſubject, and he quotes the obſervations of Richeter, with whoſe work I am not yet acquainted. (*Annales de Chimie*, tom. xxv. p. 292).

(To be continued.)

The reader will perceive that I have ſuppoſed this treatiſe to have been concluded in a former number; into which error I was led by the want of any intimation in the Annals that more was to be expected. But every cultivator of ſcience will rejoice with me that theſe reſearches, which ſhew the wonderful ſkill and acuteneſs of their author, and muſt produce an almoſt total revolution in our obſervations and reaſoning on chemical effects, are more extenſive than we at firſt imagined.—N.

VII.

On the Use of Steam as a Vehicle for conveying Heat from one Place to another. By COUNT RUMFORD.*

MORE than fifty years ago, Colonel William Cook, in a Paper presented by him to the Royal Society, and published in their Transactions, made a proposal for warming rooms by means of metallic tubes filled with steam, and communicating with a boiler situated out of the room; which proposal was accompanied by an engraving, which showed, in a manner perfectly clear and distinct, how this might be effected. Since that time this scheme has frequently been put in practice with success, both in this country and on the Continent. † Many attempts have likewise been made, at different periods, to heat liquids by means of steam introduced into them; but most of these have failed: and, indeed, until it was known that fluids are nonconductors of heat, and, consequently, that heat cannot be made to *descend* in them—(which is a recent discovery),—these attempts could hardly succeed; for, in order to their being successful, it is absolutely necessary that the tube which conveys the hot steam should open into the *lowest part* of the vessel which contains the liquid to be heated, or on a level with its bottom;—but as long as the erroneous opinion obtained, that heat could pass in fluids *in all directions*, there did not appear to be any reason for placing the opening of the steam tube *at the bottom of the vessel*, while many were at hand which pointed out other places as being more convenient for it.

But to succeed in heating liquids by steam, it is necessary, not only that the steam should enter the liquid at the bottom of the vessel which contains it, but also that it should enter it *coming from above*. The steam tube should be in a vertical position, and the steam should *descend* through it previous to its entering the vessel, and mixing with the liquid which it is to heat; otherwise this liquid will be in danger of being forced back by this opening into the steam boiler; for the hot steam being suddenly condensed on coming into contact with the cold liquid, a vacuum will necessarily be formed in the end of the tube; into which vacuum the liquid in the vessel—pressed by the whole weight of the incumbent atmosphere—will rush with great force, and with a loud noise; but if this tube be placed in a vertical position, and if it be made to rise to the height of six or seven feet, the liquid which is thus forced into its lower end will not have time to rise to that height before it will be met by steam and obliged to return back into the vessel.—There will be no difficulty in arranging the apparatus in such a manner as effectually to prevent the liquid to be heated from

* Journal of the Royal Institution I. 34.

† Although one should naturally imagine that the notoriety of these facts would have been sufficient to prevent all attempts in our days to claim a right to this invention, yet it is said that a patent for it was taken out only a few years ago.

being

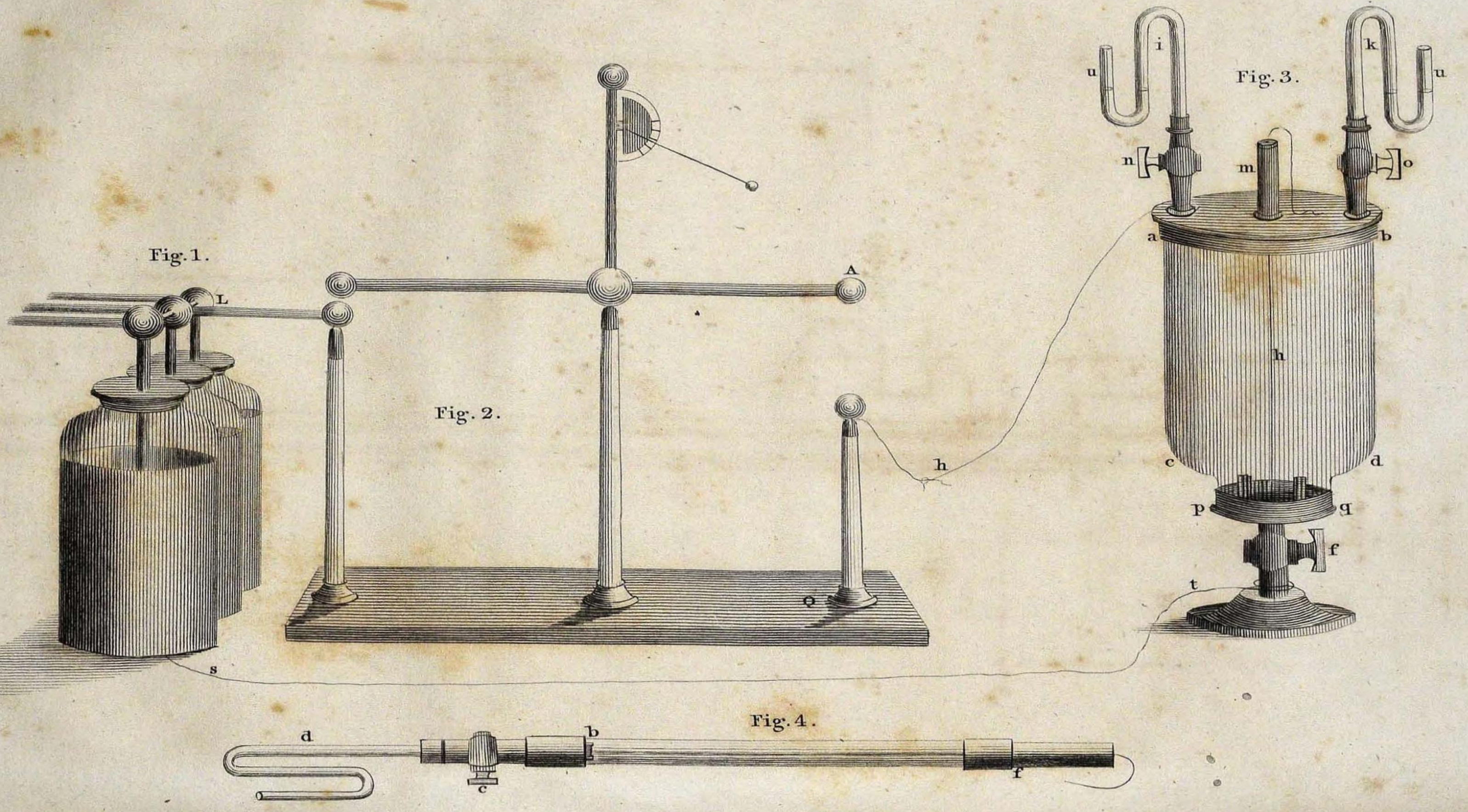
being forced backwards into the steam boiler; and, when this is done, and some other necessary precautions to prevent accidents are taken, steam may be employed with great advantage for heating liquids; and for keeping them hot, in a variety of cases, in which fire, applied immediately to the bottoms of the containing vessels, is now used.

In dyeing, for instance, and in brewing; and in the processes of many other arts and manufactures, the adoption of this method of applying heat would be attended, not only with a great saving of labour and of fuel; but also of a considerable saving of expence in the purchase and repairs of boilers, and of other expensive machinery: for when steam is used instead of fire for heating their contents, boilers may be made extremely thin and light; and, as they may easily be supported and strengthened by hoops and braces of iron, and other cheap materials, they will cost but little, and seldom stand in need of repairs. To these advantages we may add others of still greater importance: boilers intended to be heated in this manner may, without the smallest difficulty, be placed in any part of a room—at any distance from the fire;—and in situations in which they may be approached freely on every side. They may moreover easily be so surrounded with wood, or with other cheap substances which form warm covering, as most completely to confine the heat within them, and prevent its escape. The tubes by which the steam is brought from the principal boiler (which tubes may conveniently be suspended just below the cieling of the room) may, in like manner, be covered, so as almost intirely to prevent all loss of heat by the surfaces of them; and this to whatever distances they may be made to extend.

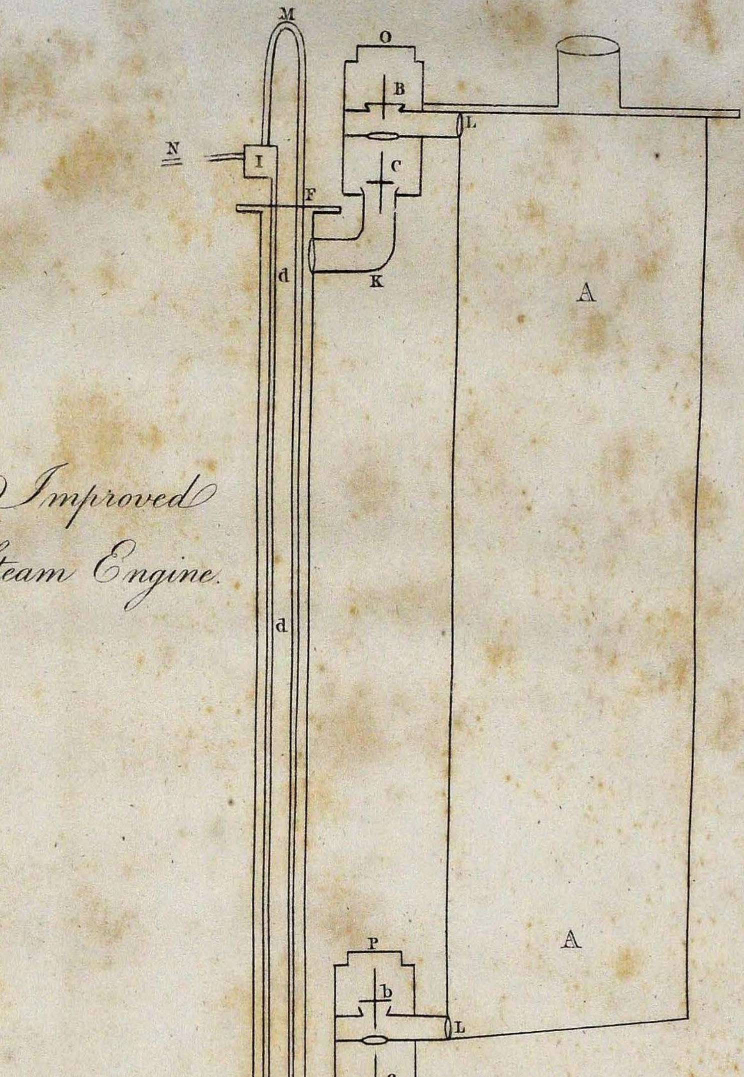
In suspending these steam tubes, care must, however, be taken to lay them in a situation *not perfectly horizontal* under the cieling, but to incline them at a small angle, making them rise gradually from their junction with the top of a large vertical steam tube, connecting them with the steam boiler, quite to their farthest extremities: for, when these tubes are so placed, it is evident that all the water formed in them, in consequence of the condensation of the steam in its passage through them, will run backwards and fall into the boiler, instead of accumulating in them, and obstructing the passage of the steam,—which it would not fail to do were there any considerable bends or wavings, upwards and downwards, in these tubes—or of running forward and descending with the steam into the vessels containing the liquids to be heated;—which would happen if these tubes inclined *downwards*, instead of inclining upwards, as they recede from the boiler.

(To be continued.)

Mr. Cuthbertson's Experiments on the Oxidation of Metals by Electricity.



Improved Steam Engine.



Rußell Court.